

Materials for a Bibliography of Translinguistic Studies

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Dr. Albert R. Bernhard,
for the author's
most sincere compliments,
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by

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Retired

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'And the whole earth was of one language, and
of one speech...'

Genesis 11:1

EXPLANATORY NOTES

One of the earliest examples of the science of comparative philology was provided by the British orientalist and jurist, Sir William Jones (1746-1794), who 'founded the (later Royal) Asiatic Society of Bengal' in 1784, in order 'to encourage oriental studies' (Haywood 1971, 13:74). In discussing the life and work of Sir William Jones, Haywood (1971, 13:75) points out that it was during the latter half of that century 'In his 1786 presidential discourse to the Asiatic Society [that] he postulated the common ancestry of Sanskrit and Greek,' which proved to herald the beginning of a new era for comparative philology. It was also during the latter half of that century that the concept and term 'Indo-European' were generated. As observed by Joshua Whatmough (1971, 12:169), the term 'is applied to a number of languages found, in the old world, chiefly in India or Europe (hence the name). The term appears to have been invented by the physician and physicist Thomas Young (1773-1829). . . . In Germany the corresponding term is "Indo-Germanic."'

Whatmough (1971, 12:169) points out that 'The Indo-European languages possess in common certain phonematic, morphomatic, syntactic and verbal (i.e., vocabulary) features which, no matter how changed in the course of time, can be explained only on the assumption that they are descended from a common source, of which we have no direct record; i.e., "by inheritance" and not by borrowing.' As Whatmough (1971, 12:169) observes, 'there is a great mass of evidence. . . not only in vocabulary but also in grammar, the interpretation of which is quite unequivocal; namely that there existed in the 3rd millennium B.C. a language, or, according to some a group of related dialects, to which the name Indo-European is conventionally applied. Its much older source is conjectured to have been in the ancient Mediterranean basin (where it may have had links with Hamitic and Semitic and perhaps Caucasian languages). . . .'

It is to be wondered at that such a highly controversial statement appears here as general information, for even today very few scholars can be found who would underwrite the theory of such 'links.' Indeed, courses in historical linguistics at universities all over the world, in spite of much perplexing evidence to the contrary, mostly still persist in adhering to strict Indo-European theories.

Yet it cannot be denied that there have always been dissenting voices, and for the better part of a century there have appeared in the comparative linguistic literature, works by scholars already well-established in the academic world by their previous scientific endeavours, who at one time or another in their lives appear to have succumbed to a sudden urge to endanger their immaculate academic reputation by indulging in what I shall term here 'translinguistic' investigations, involving 'disparate' linguistic stocks.

I am thinking here, for instance, of such internationally renowned

scholars as Graziado I. Ascoli, Franz Bopp, Linus Brunner, Björn Collinder, Aharon B. Dolgopolsky, V. M. Illic-Svityc, Saul Levin, Pierre Meille, Roy A. Miller, E. G. Pulleyblank, G. J. Ramstedt, Rasmus Rask, Edward Sapir, Aurelian Sauvageot, Morris Swadesh, C. C. Uhlenbeck, A. Wiklund, H. Winkler, and many more, as can be found mentioned in the bibliography below.

It is a well-known fact that during the last century serious comparative work has been done on such groups as (Hamito-)Semitic - Indo-European, Malayo-Polynesian - Semitic - Indo-European - Ural-Altaic - Kartvelian - North-Caucasian - Basque, including some very interesting studies adding Sino-Tibetan, Dravidian, Sumerian, and some others.

If these scholars have indeed succeeded in bringing forward sufficient evidence to warrant raising the question of the common origins purported (for true genetic relationships, four criteria being proposed as diagnostic: phonetic correspondences, shared vocabularies, common grammatical features, and identical constructive particles), we must now arrive at inferences demanding new theories.

Hence, the time seems now due to ask ourselves whether we ought not to reinvestigate and reconsider our previous concepts of 'disparate' linguistic stocks (and therefore 'cognates'), with nothing else at heart but a relentless search for scientific truth, if must be at the expense of what Koppelman (1933:2) so aptly termed 'das Heiligtum der Indogermanischen Isolierung.'

Therefore, instead of pursuing past tendencies to heap hypotheses on hypotheses on problems which obviously can never be solved within so artificially constrained a framework, we might do better to investigate the existing and known phenomena of human speech as found and seen, and try to account for these peculiar tendencies of 'totally unrelated' languages to develop similar words for identical objects, concepts and activities. Clearly, if this points to underlying theoretical principles, these should be clarified.

When attempting to do this, we find that there exist several theories, each of course with its own fiery champions and opponents.

1. Nostratic Theory

First of all there is the idea of a linguistic macrophylum, denoting long-range linguistic relationships. This was first mentioned by Pedersen 1903, who termed it 'Nostratic.' Pedersen postulated a genealogical relationship between the following languages or language families: Hamito-Semitic, Indo-European, Uralic, Altaic, Caucasian, Basque, the dead languages of Asia Minor and its neighbouring territories, as well as Eskimo, Jukagir, and the dead languages of the Bering Straits. However,

as pointed out by Korenchy (1975:109), it was only during the sixties 'that any attempt was made towards producing some sort of scientific foundation for such a type of macrolanguage family.' According to Korenchy (1975:109), 'the long pause was justified. . .because without a more thorough knowledge concerning the members of the macrolanguage family, the notion of a Nostratic family would have become degraded to only an irresponsibly playful idea.' Korenchy (1975:109) observes that when 'Illich-Svitych and Dolgopolskij settled down to work in about the middle of the sixties - to use the words of Collinder 1965b - "das fast unabsehbare nostratische Feld zu pflügen," it could already be argued that given the knowledge of the newer results, an investigation of a language relation in a wider perspective appears to be much less likely to be labelled illusory.'

Korenchy (1975:109-10) then raises the question of whether it was premature even then 'to collate several language families,' this the more so as it had been suggested by Veenker (1969:367-8, as quoted by Korenchy) that 'Illich-Svitych and Dolgopolskij were not entirely of the same opinion as to which language families belong to the Nostratic language community (Dolgopolskij earlier called it Borealis and then Euro-Siberian). According to Illich-Svitych, Hamito-Semitic, Kartvelian, Indo-European, Uralic, Dravidian, and the Altaic language families belong to the Nostratic language community. Dolgopolskij at first included Chukchi-Kamchadal, while he later left it out, but he left Dravidian completely out of consideration.' However, Korenchy (1975:110) suggests that it is the right time to research the scientific foundations for such a theory, as 'such exclusions and inclusions respectively are quite arbitrary at this initial stage of research. For what is actually the question here? The question is that once as far back as Paleolithic times the languages of the Eurasian continent could have once been members of a related linguistic community in a certain territory. From the followers of the hypothesis we could expect as a logical starting-point the a-priori argument that there was such a relationship of such an early age between all the languages, or rather, all the language families of Eurasia. Further research will decide, in the course of bringing to light the distance or proximity of the relationships between the language families, which language families should be excluded from the so-called Nostratic linguistic community.'

'Dolgopolskij regards it possible,' Korenchy (1975:110-1) argues, 'that the Nostratic theory should already be utilized in this way. It has as its starting-point the fact that the effectiveness of language comparison can be increased if many languages are compared all at once. The possibility of two morphemes corresponding by chance, as generally known, is more probable if we compare only two languages than if we do the same with more languages. On the basis of this, the method of comparing all of these language families collectively can strengthen the correspondences between the Uralic and the Altaic, the Uralic and the Indo-European, the Indo-European and Hamito-Semitic, etc., language

families, and it can also significantly reduce the number of chance correspondences.'

Korenych (1975:112) then presents an overview of the theoretical and methodological difficulties encountered in establishing a genealogical relationship between these language families, pointing out that comparative linguistic correspondences occur 'in three types: 1) ancient inheritance, 2) secondary correspondences stemming from the later areal contact of languages probably related genetically, 3) correspondences stemming from the areal contact of non-related languages'. She concludes, 'These three types of explanatory principles are similarly valid if based upon the Nostratic area.'

Finally Korenych (1975:115), while discussing her comparison of 245 Nostratic lexical items with their Hamito-Semitic, Indo-European, Uralic, Altaic, Kartvelian and Dravidian variations, points out that 'what the calculations carried out in the present material can tell us is only that we can observe in the relationships of the six Nostratic language families certain temporal or spatial, or rather temporal and spatial shifts. This point is sufficient to arouse our curiosity as to how the Nostratic area was divided and what kind of events took place there.' She concludes by observing that the 'Nostratic theory is an interesting hypothesis. Instead of the concept "language family," however, we should in the present case be satisfied with the concept "area." Future research would achieve significant results if through the help of. . .certain statistical methods. . ., other non-traditional means of paleo-linguistics, the movements that took place in the Nostratic area and the genetic type of relations that came into being there would, to a certain degree, become clear. If it could be shown which language families were in a more distant relationship with one another and which were in a closer relationship, then perhaps the internal language teachings of the language families in closer contact and which correlate with one another could also be utilized. Attempts in such a direction have already taken place, but because of their prematureness, their results are not convincing. We can await help of a different kind from a picture drawn more clearly of the Nostratic area. We can perhaps more exactly designate the individual language families, ancient areas of their diffusion, and also be able to determine the possible direction of their migrations.'

The problem of prematureness was brought up because, as the opponents of the Nostratic language theory argue, first of all a great deal of internal reconstruction work remains to be done on the individual languages within their postulated families, and secondly, because the very existence of some families, notably the Uralic and the Altaic, are still a controversial matter. Nostratic linguists argue, by contrast, that it is precisely their work which, by its very diversity, may help contribute to a speedier, more precise, more complete and more certain solution and decision in these matters. Furthermore, as they point out, their work with Proto-Turkic, Mongolian, and Proto-Tungusian (and sometimes with Korean),

makes the question of the validity of the Altaic hypothesis really irrelevant (Dolgopolsky, pers. com., Oct. '84).

Still, the broader the phylum of genetic relationships one would like to connect, the more complicated the problem becomes, because of the differences in time depth, which ultimately may prove to pose an unsurmountable handicap. Even so, the work done during the last two decades by the Nostratic linguists has put the comparative linguistic clock forward to such an extent, that it cannot be reversed and, indeed, earlier conclusions can never be the same again. Until or unless new data or techniques become available, final results can, however, because of the above-mentioned reasons, be definite only partially.

II. Monogenesis Theory

The ideas discussed above are a long cry from monogenetic theories, so popular around the end of the eighteenth and the beginning of the nineteenth centuries, and reverberations of which can occasionally still be detected today.

The monogeneticists purport a theory of the universality of human language by virtue of its single origin - a theory attacked by the polygeneticists, who argue that 'Any view which maintains a single origin or language must provide some explanation for the number of language *families* which exist' (Fromkin and Rodman 1974:21).

Fromkin and Rodman (1974:21) further argue that 'The monogenetic theory of languages - the single-origin theory - is related to a belief in the monogenetic origin of man. Many scientists today believe, instead, that man arose in many different places on earth. If this is the case, there were many proto-languages, out of which the modern language families developed.'

Interestingly, the question of the origin of man as well as the question of the origin of his language, are to this very day a subject of much scholarly debate - and sometimes unscholarly polemic. As pointed out by Wescott (1976:104), 'No discussion of language origins can avoid the question of monogenesis, that is, whether all known spoken languages have a single common origin. Oddly enough, most linguists have taken the position that, in the absence of irrefutable evidence on either side of the question, the burden of proof in the matter rests with the monogeneticists.'

As Wescott (1976:104) aptly observes, this is really very odd, as 'in regard to the analogous question of the origin of life, biologists have taken precisely the opposite position, assuming monogenesis and assigning the burden of proof to the polygeneticists.' One would, Wescott (1976:104) argues, rather 'be inclined to follow biological

precedent in this matter and assume monogenesis until polygenesis is demonstrated.' This inclination is strengthened by the fact of our well-substantiated knowledge of how the vertebrates have radiated and differentiated over space and time. It seems reasonable to infer that speech did the same thing, and that languages radiated out from a single proto-type (cf. Wescott 1976:104).

Wescott (1976:104) points out that we may further 'infer that proto-languages resembled one another more than contemporary languages resemble one another and that the earlier the protolanguages were, the more alike they were - back to the point at which they converged.' Wescott (1976:105) adds: 'Indirect evidence favoring this view comes from the documented history of well-known language families such as Indo-European. Here we find that the earliest recorded forms of most languages are patently more similar than are their contemporary forms.' These views are shared by other historical linguists such as Brunner (1969, cf. 'Einleitung,' pp. 1-17). The point where languages converged is located by Wescott (1976:104) 'in the Upper Paleolithic Period (between 20,000-50,000 years ago).' Wescott (1976:104-5) argues 'I place it before the subsequent Mesolithic Period because I believe that fewer than 20 millennia would not have produced the linguistic diversity we observe today; and I place it after the preceding Middle Paleolithic because I believe that our hominid predecessor, Neanderthal Man, though capable of manual language, was incapable of vocal language.'

Krantz (1985) observes that 'A major anatomical change occurred in the human skull at about 40,000 years ago. The change was world-wide, rapid, nearly simultaneous, and coincided with major advances in cultural complexity.' He then discusses in detail the descent of the larynx and the subsequent enlargement of the pharynx enabling vowel formation and differentiation. Although as Krantz (1985) points out '...formation of consonants ...[was] already under excellent neuromuscular control. . .,' Krantz (1985) also judiciously observes that 'Neural restructuring for rapid decoding and sound comparison would also require some time. . .' (cf. Lieberman 1975, as quoted by Krantz; Steklis 1985b).

Moreover, another problem may have been the initially but partial perfection of the pharynx. As Krantz (1985) observes, 'Linguistic reconstruction of the pre-Proto-Indo-European language indicates an absence of differentiated vowels. The date of this prehistoric language is not clear, but 10,000 years ago may well be an approximate time. Proto-Sino-Tibetan apparently was also without vowel differentiation, and this has been at least suggested for the earliest ancestor of Afro-Asian. Interestingly, the Caucasian language Kabardian today still lacks clear vowel differentiation in terms of assigning meanings to these sounds.'

Krantz (1985) further argues that 'It may be no coincidence that some of the earliest reconstructable languages lacked phonemic vowels.'

It now appears likely that these languages were spoken by people whose pharyngeal development had not yet achieved the present design. The logical steps of anatomical evolution of the speech apparatus parallel those reconstructed in historical linguistics. Neither method gives exact dates for the introduction of phonemic vowels, but linguistic methods seem the best. A good guess would be that somewhere on the order of 10,000 years ago phonemic vowels first began to come into use.' Finally, Krantz (1985) poses the interesting question of whether it may be 'possible that we have not yet evolved to the anatomical and/or neurological limit of our speech potential?' The implications of possible answers to this question are of course fascinating to ponder.

Ronen (pers. com., Nov. '84) comments: 'Among anthropologists and prehistorians, there seems to be at present no other opinion than Monogenesis. . . . By 10,000 and even 15,000 years ago, a clear Proto-Mediterranean human type, or race, existed. It is not excluded that he spoke some Proto-Semitic language. That he spoke a complex and essentially modern language is attested by the complexity of his social, cultural and economic activities. . . .'

With regard to the Nostratic Theory discussed above, monogeneticists argue (cf. Wescott 1976:108) that the shortcoming of the Nostratic linguists' hypothesis, 'according to which Indo-European, Hamito-Semitic, Ural-Altaic, Dravidian, and Kartvelian (or southern Caucasian) form a single linguistic macrophylum. . . is not that it is false - since all spoken languages are necessarily cognate - but rather that it is misleading, at least insofar as it implies that Eurasian languages have something in common with one another that they do not have in common with African, Oceanian, and American languages.'¹

However, Wescott (1976:112) is well acquainted with the Nostratic linguists' counterargument, which is that 'Although nearly all linguists, . . . , concede the genealogical unity of the Semitic, Egyptian, Berber, Cushitic, Omotic, and Chadic language groups, they have proved unable as yet to reconstruct Proto-Afro-Asian, the presumed parent language of the entire family. The same is true of Proto-Congo-Kordofanian, the presumed parent language of the Atlantic, Voltaic, Adamawa, Kwa, Bantoid, and Kordofanian groups. The chief reason for these lacunae is the predominant irregularity of sound correspondences between the recorded languages of these groups.'

'More precisely,' Wescott (1976:112) observes, 'what linguists have discovered is that, although exact phonemic correspondences between most African languages cannot be established, lexical cognations can, because corresponding phonemes in related languages usually share phonetic features. In other words, sound correspondences in these and other language families of great internal time depth must be formulated either subphonemically, in terms of articulatory or acoustic features, or transphonemically, in terms of morphophonemes.' Nostratic linguists

argue that they will only deal with what can be established scientifically, that is, according to classical comparative methodology (Dolgopolsky, pers. com., Oct. '84).

A question often asked is: What connexion, if any, does there exist between certain ethnic and linguistic groups? That is to say, after having established a genetic relationship for say, Cushitic or Chadic as Semitic languages, and for Hamito-Semitic and Indo-European as being genetically related, what implications may we infer for the peoples belonging to these linguistic groups? 'It only proves,' comments Dolgopolsky (pers. com., Oct. '84), 'that there existed, at one time or another, some contact between the peoples speaking these languages - no more, nor less.'

Finally, just as the explanation of the incredible variety of species is one of the most urgent problems of evolutionary biology, so the question of how to explain the incredible variety of languages is one of the most perplexing problems in linguistics today. Even more perplexing, however, is the question posed in this bibliography: If there exists such an incredible variety, whence the extraordinary resemblances. . .?

III. C.N.S. Theory

According to Kiparsky (1976:99), 'monogenesis is sometimes adduced as an explanation for linguistic universals.' However, there are various arguments against such an assumption. It is proposed, instead, that certain linguistic phenomena (e.g., grammatical rules) ought to be considered 'intrinsic properties of language' - that is, not necessarily one-time Universal Proto-Speech descendants, but species-specific genetic constraints that would work *whenever* or *wherever* other specifically human information is coded. It is rather here that would belong certain universals as evidenced in the child's language acquisition and other phenomena in psycholinguistic research.²

Clearly, questions concerning the origin and evolution of man as a species must of needs also concern man's most important faculty, his language, for this is, after all, a biological phenomenon with a bona fide evolutionary history, its development taking place within a well-defined biological framework (cf. de Grolier 1983; Harnad, Steklis and Lancaster 1976; Landsberg 1985; Wescott 1974). Discussing evolutionary theories regarding language origin and development, Fromkin and Rodman (1974:28) suggest that 'in the course of evolution man and language originated simultaneously, and that man is innately equipped to learn language.' Furthermore, as Fromkin and Rodman (1974:28) judiciously observe, 'Studies of the evolutionary development of both the vocal tract and the brain provide evidence for physiological, anatomic, and "mental" preconditions for language development.'

Warren, upon my request for comment, (pers. com., Nov. '84), adds: 'At the New York Academy of Sciences Symposium in 1976 (. . .) I delivered a paper in which I collected published evidence indicating that animals, although not capable of producing speech sounds, nevertheless can distinguish between closely related phonemes presented in isolation, and also when they are clustered to produce words. From this I concluded (p. 716 in the published proceedings) that ". . .it appears that human speech perception employs prelinguistic abilities shared with other animals to distinguish between phonemic groupings." It seems as if our species-specific use of speech builds upon auditory perceptual skills possessed by our ancestors long before the development of speech. The further development of the vocal tract (permitting easy and rapid generation of a variety of speech sounds) and the further evolution of the brain (permitting development of the semantic and syntactic aspects characteristic of human speech) utilized this CNS capability already in place. I believe that this pre-existing CNS capacity has guided the course taken by our evolutionary development of language.' We may wish to compare here Crystal (1973) and Steklis (1985b).

Steklis (pers. com., Jan. '85a) comments: '. . .I have no criticism to offer on your comments concerning "C.N.S. Theory." . . .There is nothing in them that I would disagree with or object to.' This seems to be a rather encouraging position.

Admittedly, the C.N.S. Theory, although it is true it can boast of promising and interesting echoes in the work of the Transformational Generativists as well as in that of the Language Universalists, is still in its infancy, with as yet sparse concrete evidence. However, a more rational and scholarly approach to various existing problems as discussed above, will hopefully in due time guide us to a more realistic, more real appraisal of the genealogy of the languages of the earth. 'Time' is a key-work here, for we cannot measure the extent of duration present and future researches will take, and much patience and perseverance are called for. Perhaps it will be given to a coming generation of young scientists to find out that where omnicomparatism fails, biology may step in. And it may eventually be found to be so that this perplexing panlinguistic network of 'similarities' has something to do with the species-specific constraints on the variables of the electro-chemical pattern making ability of the human mind.

Such an approach may help us view the enigma of the human animal's acoustic signal somewhat less pessimistically and eventually yield a model accounting for the nearly infinite variations on the common theme of our humanity.

NOTES

¹It may here be noted that Basque is mentioned by neither, although there exists a vast literature dealing with this problem (see this bibliography). Dolgopolsky (pers. com., Oct. '84) comments that Basque is not included in Nostratic theory because not enough internal reconstruction work has, as yet, been done on it, and that, moreover, it is possibly a very ancient offshoot of Hamito-Semitic.

²Here belong perhaps also such phenomena as cited by Wescott (1976: 114), '. . .the kind of improbable coincidence that Carl Jung called "synchronicity."'

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